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THE NEW OPPORTUNITY IN SCIENCE[1]

SINCE I had the good fortune to be somewhat intimately associated with many phases of scientific development work in this country in aid of the war, and also had exceptional opportunity, through reports which came weekly from the scientific attachés in London, Paris and Rome, to become familiar with similar developments in Europe, you will expect me to see the new opportunity in science in situations created by the war or in lessons taught by it. That expectation I shall endeavor not to disappoint. I shall accordingly introduce my subject by a brief review of the most noteworthy features of the methods employed and the results obtained in applying science to the needs of the great war.

That you may be under no misapprehension, however, regarding the importance of the rôle which I myself have played in these events let me begin with an incident of the summer of 1917. It was in the last week in March, 1917, that I gave up my academic duties and was called to Washington as vice-chairman of the National Research Council, charged particularly with the task of assisting in mobilizing the scientific men and the scientific facilities of the United States in aid of the war which was clearly coming, although it had not yet been declared. During the hectic months of the spring of 1917, when the civilian activities in aid of the war were directed by the Council of National Defense in the Munsey Building, I represented the Research Council upon the old Munitions Board and also

[1] A lecture given on July 25 before the summer session of the University of Chicago.

as the representative of the Research Council was appointed by the Secretary of the Navy one of the four advisory members of the Anti-submarine Board, which was the board charged with the direction of the anti-submarine experimenting in the United States. A little later, when it was finally decided that the supply and development work for the American Army was not to be carried on as in England by a civilian organization headed by a minister of munitions, but was to be conducted by the bureaus and corps of the Army itself, like many others who had been intimately associated with the work of the Council of National Defense I was placed inside the Army and given charge of the so-called Science and Research Division of the Signal Corps. I had held this office for about a month and had been passing back and forth in the Munsey Building in a major's uniform, when one morning there appeared in the Washington *Post* an editorial entitled "The Unconquerable Spirit." This editorial was inspired by the appearance of two books, of one of which I had been the unfortunate author, while the other was from the pen of Professor Henry Fairfield Osborn. The one dealt to some extent with the evolution of matter, and the other with the origin and evolution of life on this earth, and the editor having had his attention directed to these two books sat him down and opined about as follows: "Here is a world at war and yet there are found two detached, imperturbable souls, one of whom is still dreaming about the origin of matter and the other about the origin of life, all unconscious of the cataclysmic events which are taking place in the world in which we live." This editorial was penned in the building adjoining that in which I had been going back and forth in soldier clothes for as much as a month and in civilian clothes for three months more. Mrs. Millikan, whose pride was more or less touched by the incident, suggested that the editorial was improperly named, that it should have been entitled not "The Unconquerable Spirit" but "The Contact of the Press with Reality" or "Asleep on the Post." I am sure I should be glad to accept the emendation and I presume Professor Osborn would also, if the editor of the *Post* would agree. But in any event the incident gives an altogether correct picture of the conspicuousness of the rôle which I played in Washington affairs during the period of the war.

A wise man learns even more from his failures than from his successes. One of the most dismal failures of the war was made in the endeavor by all of the principal belligerents to utilize the inventive genius of the *average citizen*. Every major belligerent had a board of inventions and research to which every man with an idea was asked to communicate that idea. All of these boards had precisely the same experience, in England, France, Italy and the United States. They all agree that not one suggestion in ten thousand which came in in this way was of any value whatever, and that the occasional worth-while idea which was presented to these boards was in general arrived at earlier in other ways. It may then be set down as a fact fairly well established by the experiences of the Great War that rapid progress in the application of science to any national need is not to be expected in any country which depends, as most countries have done in the past, simply upon the *undirected* inventive genius of its people to make these applications.

And yet every one of the aforementioned countries actually did make during the war extraordinarily rapid progress in applying new scientific methods to the problems of submarine detection, of aviation, of signalling, of gas warfare, of me-

chanical warfare and of the location of enemy guns, airplanes and mines. How was it done? What was the method adopted to stimulate development in such an extraordinary way as it was stimulated during the Great War? Let me answer by relating a single chapter from our own experience, which is not only representative of all American experience in this field, but is also similar in essential particulars to the experiences of the other countries mentioned.

On March 3, 1917, two days before the United States had declared war, the Military Committee of the National Research Council, consisting of the heads of the principal technical bureaus of the government, both military and civil, and the chairman and vice-chairman of the council, met at the Smithsonian Institution in Washington and dispatched at once a Scientific Mission to Europe to ascertain by first-hand contact and to report back to the United States the exact status at that time of scientific development work in Europe in aid of the war. This mission was received with open arms by the Allies, for it arrived at the darkest hour for France in the history of the war, namely, the hour following the disastrous attempts which General Petain made to push back the German lines in the spring of 1917.

The French government, headed at that time by M. Painlévé, himself a scientist, not only gave the seven scientists, Messrs. Ames, Burgess, Hulett, Williams, Dakin, Reid and Strong, who constituted this mission, opportunity to come into intimate contact with all scientific developments under way or projected at that time, but he arranged to have a return mission, consisting of some of the most eminent of French, British and Italian scientists, such as Majors Fabri and Abraham, le duc de Guiche, and Professor Grignard from France, Sir Ernest Rutherford and Commander Bridge from England, Lieutenant Abetti from Italy, sent back to this country with definite official instructions to hold back nothing, but to lay all the facts and plans of the Allies relating to scientific developments in aid of the war before properly accredited scientific men in the United States.

The National Research Council, which acted as the hosts of this mission in the United States, with authority conferred upon it by the War and Navy Departments, called a conference in Washington of some of the best scientific brains in the United States and for a period of a full week this conference met and discussed in detail the progress thus far made and the plans projected in the fields of submarine detection, of location of guns, airplanes and mines by sound, of ordinance, of signalling, of aviation instruments and accessories, and of chemical warfare.

As a result of these conferences there were organized through the cooperative effort of the National Research Council and several of the bureaus of the Army and Navy, a considerable number of groups of scientific men, each of which was charged with the development of some particular field. For example, Professor Trowbridge, of Princeton, and Professor Lyman, of Harvard, were selected and placed in charge of the development in this country of the sound-ranging service. They and the group of scientific men whom they associated with them were first given commissions in the Signal Corps, and with Signal Corps authority and funds started development work in sound-ranging at Princeton University and at the Bureau of Standards. This whole group was later transferred to the authority of the Engineer Corps, but its directing personnel remained in the main unchanged and it

did extraordinary work in the whole of the fighting of the summer of 1918, locating hundreds of guns by computing the center of the sound wave from observations made on the times of arrival of the wave at from three to seven suitably placed stations. This method had never been used in any preceding war and it proved extraordinarily accurate, a gun being located five miles away with an error of less than fifty feet.

Again it is not an over-statement to say that the most effective of the anti-submarine work done in the United States grew directly out of that conference, and it grew out of it in this way. As Lord Northcliffe continually reiterated on his trip to the United States in the spring of 1917, the submarine problem *was* at that time *the* problem of *the war,* for while Europe might fight with little to eat, it could not fight without iron and oil and other supplies which this country alone could furnish, and in the spring of 1917 civilization trembled in the balance, because the submarine was seriously threatening to destroy all possibilities of transportation from this country to Europe. The English scientists therefore, in particular, came to this country directed by their government to lay before the American scientists every element of the foreign anti-submarine program, whether already accomplished or merely projected, and in the conference under consideration a large part of the discussion centered around the submarine situation. Now the problem of submarine detection, as Sir Ernest Rutherford repeatedly pointed out, was a problem of physics pure and simple. It was not even a problem of engineering at that time, although every physical problem, in general, sooner or later becomes one for the engineer, when the physicist has gone far enough along with his work. Hence, the number of physicists being quite limited, the number of men who had any large capacity for handling the problem of anti-submarine experimentation was small. These men existed mostly in university laboratories or in a very few industrial laboratories which employed physicists, and we unquestionably had gathered a very representative group of them together in the fifty men assembled in the conference at Washington. The success or failure of our anti-submarine campaign, and with it the success or failure of the war so far as we were concerned depended upon selecting and putting upon this job a few men of suitable training and capacity.

At the close of that conference a small committee was appointed to select ten men to give up their work and to go to New London to work there night and day in the development of anti-submarine devices. The men chosen were Merritt of Cornell, Mason of Wisconsin, H. A. Wilson of Rice Institute, Pierce and Bridgman of Harvard, Bumstead, Nichols and Zeleny of Yale, and Michelson of Chicago, although Professor Michelson was almost immediately taken off for other work of much urgency and Chicago was represented in a fashion by the writer who was there a portion of each week. This group worked under the authorization of the Secretary of the Navy and with the heartiest of cooperation from the Navy Department, although it was at first financed by private funds obtained by the National Research Council. In the course of a few months, however, when it had demonstrated its effectiveness it was taken over by the Navy, which spent more than one million dollars on the experimental work at that place. This station with its chief scientific personnel not largely changed became the center of our anti-submarine activity, and with other stations, one at Nahant, Mass., embracing

chiefly the physicists of the General Electric Company, the Western Electric Company and the Submarine Signaling Company, one in New York presided over by Dr. Pupin, of Columbia, and one in San Pedro, Calif., which, like the New York station, was organized under the Research Council, made remarkable progress in the rapid development of anti-submarine devices—devices which exerted a notable influence upon the reduction of submarine depredations, and made it possible even by the fall of 1917, to predict that the submarine menace could be eliminated. Unquestionably the most effective device developed in America, and one which played a real rôle in the elimination of that menace, was one which grew immediately and directly out of the above-mentioned conference. The French had already developed an apparatus consisting of a sort of great sound lens which brought the incoming pulses together in the same phase at the center of the lens near the bottom of the hull. This was presented and discussed at length in the conference. A full official report of the device was sent by the French government to the Anti-submarine Board of the Navy, and at a meeting of that board the writer requested to be allowed to take this report to the group of scientists at New London for the sake of a thorough analysis of it, for he felt confident, and so stated at the time, that through such an analysis we would obtain variants of the device which would be an improvement upon it. This procedure was followed and for two days ten men assembled at a hotel in New London and studied that report, drawing up four or five different variants of this device to develop and try out. The most successful and effective detector which actually got into use in the war was one of these variants of the original French device. Many of our submarines and destroyers which went across during the summer of 1918 were equipped with it, and now it is being still further developed for peace use, rather than for war, for it is possible through it to eliminate the chief terror of the sea, namely collision in fog. And, when it is remembered that the preventing of a single disaster like the sinking of the *Titanic* or of the *Empress of Ireland* more than pays, without any reference to the value of human lives, for all the time and money spent by England, France and the United States combined in developing detecting devices, it will be seen how shortsighted a thing it is for any country to fail to find in some way the funds necessary for carrying on research and development work in underwater detection. For decades and for centuries we have allowed ships to go down year by year needlessly, simply because we have not realized the possibilities of prevention through properly organized scientific research in this field.

But it has not merely been in sound ranging and in submarine detection that the war has demonstrated the capabilities of science. Every single phase of our war activities has told the same story. Turn, for example, to the development of new scientific devices for use with aircraft. How was that handled? The Science and Research Division of the Signal Corps, organized through the cooperation of the Signal Corps and the National Research Council and later transferred to the Bureau of Aircraft Production, had a group of as many as fifty highly trained men, physicists and engineers, who were working in Washington and in the experimental station at Langley Field, twelve hours a day, seven days a week, on aviation problems—one group on improvements in accurate bomb dropping, another on im-

provements in airplane photography, another on the mapping of the highways of the upper air in aid of aviation, another upon balloon problems, such as the development of non-inflammable balloons concerning which you have read in the papers, another on aviation instruments, compasses, speed meters, etc., and producing the best there are in the world, and finally a chemical group on new sources for acetone for airplane coverings, new sensitizing dyes for long wave-length photography, etc. Let me select for special comment but one or two of the seventy odd problems which these groups alone were actively engaged upon at the signing of the armistice.

Throughout the whole of the war bombing was done in a very inaccurate, a very hit-or-miss way. At Langley Field a group of able scientific men were set upon that problem—and there are only a few men in the country who have the requisite training for handling the difficult problems of stabilization which are here involved. That group, headed by Dr. Duff, improved the accuracy in bombing so far as the main error was concerned, which is in the determination of the vertical, by more than three-fold, and when it is remembered that a three-fold increase in the accuracy of placing bombs is exactly the equivalent of a three-fold multiplication of the production of bombing planes, it will be realized how important it is to devote the small funds necessary to get scientific men to solving these problems, and not to confine attention merely to the problems of production.

Or, take again the problem of airplane photography. The developments of the war have completely revolutionized the whole art of surveying, for a camera in an airplane can now take in a few seconds a complete map of any locality, even from a height as great as 25,000 feet. It is only necessary to have a few fixed points on the photograph which are determined by the old triangulation methods and, by simply measuring up the photograph you have all that it used to take years of time to get by the old-time methods. Probably the finest airplane cameras in existence were developed by the American group assigned to that task.

Or, look at the work of the Meteorological Section of the Science and Research Division. It developed long-range propaganda balloons, capable of flying more than one thousand miles in the upper regions of the air where the prevailing winds are practically always from west to east and have speeds of 30, 40, 50 or even 100 miles an hour. It also mapped these upper regions in aid of aviation, an undertaking the importance of which can be seen from the fact that an aviator above the clouds who knows nothing of the direction of the winds will move toward his objective 200 miles an hour faster if he is helped by a 100-mile wind than if he is opposed by it.

These are merely samples of the results which were obtained in extraordinarily rapid time by the group-method of attack upon the scientific problems of the war, and these are merely a few of the developments which came under the writer's immediate attention.

In the Chemical Warfare Service equally rapid and equally important work was done in the development of new gases and in the development of means of absorbing enemy gases in gas masks. I am quite happy to be able to say that Professor Lamb, who was at the head of the Offensive Gas Warfare activity, has assured me that the key to the development of the American gas mask came from the work which Dr. Lemon has for years past been

carrying on at Ryerson Laboratory on the absorption of charcoal. In the Ordnance Department too under the leadership of Professor F. R. Moulton, of our department of astronomy, new methods of computing the trajectories of projectiles were developed in collaboration with the meteorological service, already referred to. New sets of range tables were devised which included corrections for the so-called ballistic wind. Without such corrections which are altogether new in artillery practise, firing by the map, which, in view of the development of long-range guns and camouflage, represents a large fraction of all firing, becomes utterly impossible, for the ballistic wind correction would often make a difference of a half a mile in the point of landing of the projectile. When it is remembered that the biggest element in the effectiveness of a modern army is its artillery, and that the effectiveness in the artillery is dependent entirely upon the accuracy of these wind corrections, it will be seen how incalculably valuable the work of the trained physicist and mathematician has proved to be to the practical problems of modern war.

Do not, however, let me give the impression that our groups in this country have been more successful than have corresponding groups in England and France. The general method of attack has been the same in all these countries, and the experimental groups in them all have functioned as a unit through the development of the so-called Research Information Service, which was financed by a grant of something like $150,000 which the President gave from his emergency fund to the National Research Council for the establishment of four offices, one in Washington, one in London, one in Paris and one in Rome. The office in Washington was headed by a group of three men: the chief of the Army Intelligence Service, the chief of the Navy Intelligence Service, and the chairman of the National Research Council: the group in London by the naval attaché, who is Admiral Simms himself, the military attaché, and a new appointee called the scientific attaché, chosen by the National Research Council. The function of the scientific attaché in England was to keep in touch with all research activity in that country and to send back almost daily reports to our office in Washington. Similarly, all reports of work done on this side were sent by uncensored mail or by cable to the offices of the scientific attachés in London, Paris and Rome and distributed from there to the research groups in Europe. At the request of the General Staff, the Secretary of War issued orders to all army officers who were sent on scientific and technical missions to make duplicate reports, one to the officer who sent them and the other to the office of the scientific attaché, so that there might be a central agency through which an interconnection might be had between all kinds of new developments.

Furthermore, through the authority conferred by the Military Committee of the National Research Council, embracing the heads of the technical bureaus of the Army and Navy, Admirals Benson, Griffin, Earl Taylor and Generals Williams, Squier, Black and Gorgas, there was held in Washington at the office of the National Research Council a weekly conference which reviewed all the reports from abroad each week and put the workers on this side into the closest touch with the developments on the other side. The whole plan was an admirable illustration of the possibilities of international cooperation in research. In the submarine field, for example, all anti-submarine work in England, France and Italy which was reported by cable and

by uncensored mail immediately to the office of the Research Council in Washington, was taken each Saturday night to New London and presented in digested form to the group of scientists which was working there continuously on submarine problems. Similar arrangements were made with the aeroplane research groups, sound ranging groups, etc., so that in the Research Information Service we had the first demonstration in history of the possibilities of international cooperation in research on a huge scale, a sort of cooperation which made it possible for any develment, or any idea which originated in any of the chief civilized countries of the world to go at once, very frequently by cable, to all the other countries and to be applied there as soon as possible, or to stimulate carefully selected groups of competent technical men in these countries to further development. *The extraordinary rapidity with which scientific developments were made in the war was unquestionably due then, first, to the forming of these highly competent research groups, and second, to the establishment of effective channels for the coöperation between these groups.*

But what have all these accomplishments of science in the war to do with the new opportunity in science? Simply this; for the first time in history the world has been waked up by the war to an appreciation of what science can do. Why have we gone on for hundreds of years wasting millions and hundreds of millions of dollars in collisions between ships? Why have we not years ago in times of peace gone at the problems of under-water detection in the way in which we went at them during the war? Simply because men in authority have been asleep to the possibilities. But now for the moment at least they are awake. How long they will remain awake is problematical. But just now the war has taught our political and industrial leaders what science can do. The war has also taught young soldiers that they need their science for success. Administrative positions in the industries are to-day being filled as never before from the ranks of the technically trained men. The war has taught the prospective officer that he can not hope for promotion unless he has scientific training. The war has taught the manufacturer that he can not hope to keep in the lead of his industry save through the brains of a research group which alone can keep him in the forefront of progress.

As a result of all this there is indeed a new opportunity in every phase and branch of science. There is a new opportunity, first, for science in the secondary schools. I hope, at least, that we are going to have an awakening among our principals, superintendents and educational leaders which will make it possible pretty soon to get consecutive, systematic, thorough work in science in the high schools. This is simply a matter of school *administration* and *organization*, and I hope some time principals and superintendents will wake up to that fact. So long as they continue to do what is called in the service "passing the buck," and put it all up to the teacher of science who is absolutely helpless without them no progress can be made. So long as our elementary science is taught by what I choose to call the pellet method of instruction, by which I mean that the science is split up into yearly or half-yearly doses without antecedents and without consequents, we shall never have worth while training in science in the public schools, no matter what the angle of approach, or what the arrangement of subject-matter. The crying need is not for a reorganization and rearrangement of the subject-matter of science. That has been

done and redone every year for twenty years to no avail and it will continue to be done to no purpose until we get a reorganization of the curriculum which makes it possible for the same group of students to get say three *continuous* years of science. The General Science movement has unquestionably thus far been a step backward, rather than a step forward, for it has intensified the pellet-science evil instead of eliminating it. It is acceptable to principals and superintendents simply because the easy thing from an administrative standpoint is to have no continuous courses at all, and we have in recent years been doing for the most part in our school organization the easy thing instead of the pedagogically sound thing. There is then a new opportunity and a tremendous one for those of us who are connected with the teaching and administration of science in the high school.

Second, there is a new opportunity for the application of science to the industries, for the war has demonstrated in the ways which I have indicated, the effectiveness of work of groups of well-trained scientific men, and the leaders of our industries are awakening to that fact, and they are now forming such research groups. Three large manufacturing establishments have written to me within a month, saying that they were starting departments of physical research in connection with their industry and they wanted highly competent physicists to man those departments. The Ph.D. in physics, if he is a man of ability, is in demand to-day in the industries as he has never been before.

Third, there is a new opportunity for the established scientists in the development of the possibilities of cooperative research among themselves. Most of the work in science in the past has been done by the individual, isolated experimenter. The war has demonstrated the immense advantage of cooperation between research groups even though they be in different countries, and the National Research Council is making vigorous efforts at the present time to open up the possibilities of cooperative research even of an international kind. Under the stimulus of that body there is being formed this week in Brussels a new International Physical Union, a new International Geophysical Union, a new International Astronomical Union, a new International Chemical Union. In this country the Physical Science Division of the National Research Council has divided up the field of physical research into twenty departments, has assigned a group to each department, and has found a way by which we can try out the possibilities of cooperative research in physics, by getting most of the workers in each field together once or twice a year for the sake of comparing notes, analyzing the whole situation, eliminating as far as may be duplication, and starting new work on fields that do not seem to be adequately covered by work already under way, and in general stimulating one another by mutual contact. This, I take it, is one of the great opportunities in science at the present moment, and I anticipate great results from the introduction of this method.

Fourth, there is to-day a new opportunity in science for the young American who is facing the problem as to where his life can be spent on the whole most effectively. It is to be assumed that most men are at bottom altruistic, that most men seek to direct their lives into channels in which they can make them most worth while for the race. I should like to divide all altruistic effort into three great classes:

The first has to do with efforts toward the improvement of the individual characters and lives of men. This is the field

which for thousands of years has been the chief concern of religion, and it is perhaps the most fundamental and most important of all. Its needs and its opportunities are eternal, and no thinking man leaves it out of account. But it is not this field to which I am directing attention to-day.

The second type of effort has to do, in one form or another, with possible and projected changes in the distribution of wealth. In this category are found all efforts toward social rearrangements, and educational reform, brought about either by legal enactment, or by the development of an enlightened public opinion. No man in his senses would belittle this type of effort. The needs are tremendous and every right thinking man bids every worker of this sort godspeed. This is, however, the field in which most of the moot questions exist and in which most of the big mistakes are made. Moreover, this is the field which is always before the public eye, and which absorbs nine tenths of the nations' capacity for discussion in print or on the platform.

But it may after all be questioned whether effort in this field has as good a chance—I had almost said one tenth as good a chance—of effectiveness in contributing to human well-being as has effort in the third field, namely, the field which has to do broadly with the creation of wealth rather than with its distribution. This last is the field of scientific and engineering endeavor; for the scientist is, in the broad sense, a creator of wealth as truly as is the man whose attention is focused on the application of science. Indeed, the scientist is merely the scout, the explorer, who is sent on ahead to discover and open up new leads to nature's gold. His motive may be merely to find out how nature works, but once that knowledge has been gained, man almost always finds a way to apply it to his own ends, so that in a very real sense all scientific effort is directed toward the improvement of human well-being through the creation of more wealth.

Now it goes without saying that it is impossible to distribute more than is created, and where the wealth is once created there is no little evidence that natural processes in the long run do a good deal, at least in democratic countries, toward producing a more or less reasonable distribution. The inequalities and injustices which strike the eye are of much less general significance than the superficial observer realizes. A progressive economist told me the other day that I was probably making an overestimate when I stated that a complete levelling of all incomes in the United States might possibly increase the income of the average worker by 10 per cent. I am informed by one who is in position to know the facts that such a complete levelling in the telephone industry, for example, could not increase the average income of the wage earner by more than 2 or 3 per cent., and I have been given, from what I consider fairly reliable sources, about the same figures for the steel industry. It is probable that the total possibilities of improvement of conditions through changes in distribution are very limited, while possibilities of improvement through increase in production are incalculable. But whether rough figures and estimates like the foregoing have any value or not, this much may be set down as certain. The present distress in Europe is not due to bad distribution but simply to lack of production. Equally certain it is that no one who visited Europe frequently before the war and came back to this country, as I have often done, with the observation that here one finds in comparison with Europe large comfort, large intelligence, large well-being in the case of the average

man, will claim that the prosperity and comfort of the average American citizen as compared with his European brother is due to a better mode of wealth distribution which is in use in this country. Our critics claim that we have the worst system of distribution in the world, since it is here that the great fortunes are piled up. *There can be no question that the better wage and the greater prosperity of the American workman is due primarily if not wholly to the fact that the American workman in every line of industry actually produces from two to five times as much per man-hour as does his European brother.* The reasons for this fact need not concern us here. They lie partly no doubt in our national resources, partly in a spirit of accomplishment which has been created here, and partly, though not wholly, in our use of labor-saving machinery. But it is the fact and the obvious consequence of it in the increased opportunity and well-being of the average man to which I would here call attention. *How unimaginable then the stupidity and how pathetic the blundering of that large class of labor leaders who are endeavoring to improve the conditions of labor by limiting production.* Such efforts can only bring disaster. If successful they merely result in robbing one class at the expense of another, and the robbed class is, in general, the one which is already least favorably situated.

However important, then, the problems of distribution may be there can be no uncertainty about the even greater importance of the problems of production. One little new advance like the discovery of ductile tungsten, which makes electric light one third as expensive as it was before, is a larger contribution to human well-being than all kinds of changes in the social order. The man who finds a way to so harvest his hay as to make a given plot of ground feed twice as many cattle as it did before has contributed immeasurably to human welfare. So has the biologist who shows mankind how to defeat the law of Malthus and to propagate rationally instead of in accordance with the law of the jungle. Or again the pure scientists who for ten years worked out the properties of discharges of negative electricity through highly exhausted bulbs and so made possible the use of pure electron discharges in multiplying enormously the possibilities of telephonic and telegraphic communication—the cornerstone of international good will—have made their lives count for humanity as very few political or social reformers have ever been able to do. These are the sort of opportunities which lie before the young man who is now choosing his life work in science, and incomparable opportunities they are.

Imagine a country which is made up of hills and valleys and in which the valleys often become flooded so as to drown out the valley-dwellers. A part of the people of this country set to work to level down the hills and fill in the valleys so that all the inhabitants may live in safety. These are the political and social reformers. And another part, without attempting to interfere with the topography, set themselves the task of raising the whole level of the land or lowering the level of the water so that the danger of floods is altogether gone. These are the creators of new wealth, the scientists and engineers. Both groups are needful to progress, but I suspect that the second group is less likely to make costly mistakes and more likely to accomplish useful results than is the first group. Neither group, however, should slacken its effort.

Fifth, there is a new opportunity in science for the man who wishes to invest his

wealth so that it will yield the largest possible returns to his country and his race. The United States has not in the past been the leading scientific nation, it can not even claim to have been on a par with two or three of the foremost scientific nations, at least if population is considered in assigning places. The number of outstanding scientists whom we have thus far developed in this country has not been at all proportionate to our population. This I take it is not because we do not produce as able men as any nation, but because our ablest men have not gone in large numbers into scientific activities. Why? Simply because the public appreciation of science has not been strong enough; because the rôle which science plays in the march of progress has not in the past been sufficiently generally realized among us; and because we have not developed centers of scientific research in which the atmosphere of research can be breathed by the young American who is about to choose his life work. If anything has been demonstrated by the history of the last century it is that that nation which is in the forefront in scientific developments is the nation which is going to lead in commerce and in industry and in every other phase of human activity. For who with eyes to see and ears to hear and a brain to consider can doubt for a moment that the keynote of modern civilization lies in the control of nature's forces by man, and who can doubt that that country which ferrets out nature's secrets most successfully will be the country which controls those forces most effectively? What then can be done to make this country utilize its tremendous natural advantages to the full and play the rôle which it ought to play in the progress of science and the world? I might answer in terms of the programs of the nations which have been stimulated by the war to the development of new programs of scientific research. Great Britain, Canada, Australia, Japan, have all recently made large governmental appropriations in aid of research in the physical sciences. Some of them are founding with these funds great research institutes, as Germany did before the war. Efforts of this kind are not to be decried. They will undoubtedly serve a useful purpose. But they are not in themselves adequate to our American situation. The mode of approach is not that which conforms best to the genius of our institutions or which the experience of the past indicates is likely to yield the largest returns. Furthermore, we are already in many places in this country over equipped with facilities and under equipped with men. Every purely research laboratory, whether under the control of the government or of an industry, is in the first instance a man-consuming rather than a man-producing institution. Our greatest need is not for more facilities, but for the selection and development of men of outstanding ability in science. Find a way to select and develop men and results will take care of themselves. This need for the development of men can be met only by the American universities. But it can not be met even by them unless we check in some way the tendency, met because of the growth in numbers in all our universities, for instruction to encroach upon and crush out research. The stimulus to research which comes from its association with advanced instruction is unquestionable and the broadening influence of a university is perhaps well-nigh essential to the best growth of the scientific mind. Hour for hour research in universities is probably much more effective than research in detached research laboratories, but the difficulty is that the number of hours available in most of our universities is still pitifully small.

The universities can not possibly fulfill their function of selecting and developing scientific men of outstanding ability *unless they create within themselves the atmosphere of scientific research.* The creation of research men may not be the prime function of all universities but it should certainly be the prime function of some of them. One of the most urgent needs then of America to-day is for the development in connection with five or six American universities of great research institutes in the natural sciences, such as do not exist at all to-day, institutes in which there will be as many able investigators devoting two thirds of their time and energy to research as are now found in the detached research institutions like those of the Carnegie Institution and the Rockefeller Institute for Medical Research, or the research laboratories of the Western Electric, General Electric and the Westinghouse companies. It is believed that such institutes in connection with American universities, where they will be freed from the limitations of industrial laboratories, divorced from the narrowing influences of detached research institutions, so placed that the research atmosphere which they create can be breathed by the most talented youths who pass through our American educational system, will exert a very marked influence upon the development of preeminent scientific men in America and upon making this country a center of the world's scientific life and progress. How can such institutions be created? Perhaps by government initiative. But if we may argue from the past the development is likely to come about in America in another way. We have developed in the United States a highly patriotic and highly intelligent public sentiment which stimulates men of wealth and power to devote themselves and their fortunes to great public enterprises. No country in the world has developed such groups of private individuals who hold their talents and their wealth as public trusts. Most of our great advances in the past have been through private initiative, and I suspect Mr. Elihu Root was as usual a wise counselor when he said recently in substance, "If we are going to conserve the finest elements in Anglo-Saxon civilization, we must conserve the method of free private initiative and not depend primarily upon government aid." The great opportunity in science then for the man who wishes to invest his funds where they will count most for his country and his race lies in the endowment of research chairs, or better semi-research chairs, in a few suitably chosen educational institutions. Such monuments ought to be infinitely more attractive than those of brick and stone. Such a chair endowed in such a way as to attract the ablest men whom we develop and filled continuously by fertile men will yield bigger returns to the donor and to the world than any other investment which can be made. Therein lies the greatest opportunity which America offers to the philanthropist to-day.

If some such program as I have outlined for producing scientific men and for creating centers of research in connection with a few American universities can be adopted in the United States, and I think it will be, then in a very few years we shall be in a new place as a scientific nation and shall see men coming from the ends of the earth to catch the inspiration of our leaders and to share in the results which have come from our developments in science. If we fail to seize these opportunities then the scepter will pass from us and go to those who are better qualified to wield it.

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